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Source: *American Sociological Review*, Vol. 44, No. 5 (Oct., 1979), pp. 843-850
Published by: American Sociological Association
Stable URL: http://www.jstor.org/stable/2094531
Accessed: 12-01-2016 15:21 UTC

A PANEL MODEL OF CRIME RATES AND ARREST RATES*

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In this paper, panel models for crime rates and arrest rates are utilized to separate the effect of law enforcement from several other processes that have been advanced as possible determinants of an enforcement-crime relationship. When models of this type are estimated for official index crime rates in a sample of U.S. cities for the years 1964–1970, no meaningful relationship between arrest rates and crime rates is found. This finding permits us to exclude the existence of any appreciable deterrence effect.

A decade ago, research interest in crime deterrence was revived by two studies that reported significant negative relationships between geographically aggregated crime rates for some index offenses and measures of the certainty and severity of imprisonment (Gibbs, 1968; Tittle, 1969). Since then numerous investigators have continued to interpret correlations between crime rates and various measures of law enforcement activity as evidence for the existence of a deterrent effect.[1] It is equally plausible, however, that these correlations reflect the effect of crime rates on law enforcement, or the effect of other variables on both rates.

The impact of crime on enforcement practices can come about in at least two ways. (1) Higher crime rates could tend to saturate crime control capabilities that are fixed in the short run. Thus when crime rates increase, police resources may be stretched thin, reducing the probability of an arrest following an offense. When the number of arrests increases, prosecutors and judges faced with larger caseloads may dismiss more cases and accept plea bargains more favorable to the defendant. If the number of commitments to prison leads to overcrowded prisons, parole authorities may release prisoners earlier than usual, reducing the average length of sentence served in prison. (2) Crime rates could influence enforcement through their effect on attitudes toward crime. It is possible that high crime rates create habituation to crime and hence lower penalties; on the other hand, high crime rates might tend to increase public fear of crime and thus lead to more punitive or efficient enforcement measures.

It is important to note that these two potential effects of crime on enforcement differ not only in the time span over which they are likely to be felt but, more importantly, in their signs as well. The saturation effect, which will tend to reduce enforcement efficiency as crime increases, will be felt almost at once, while the influence of the crime rate on public demand for punishment could tend to increase enforcement efficiency as crime rates rise. However, this effect will not be felt immediately, but will lag behind the crime

[1] For a recent review of this voluminous literature see Nagin (1978).

rate, both because public recognition generally lags behind "objective" indicators of social problems, and because it usually takes time for public concern to be translated into enforcement policy.

As for the impact of enforcement practices on crime, the period of time over which the influence is expected is uncertain. The deterrent, restraining, rehabilitative and stigmatizing effects of enforcement could all occur on both a short-term and long-term basis. Here, however, there is less reason to expect these effects to be of opposite sign.

Alternative Methods of Estimating Reciprocal Influences

All but a handful of studies of the effect of law enforcement on crime have employed cross-sectional designs; the more sophisticated of these studies have used simultaneous equation methods for separating the effect of crime on punishment from the effect of punishment on crime (Ehrlich, 1973; Greenwood and Wadycki, 1973; Mathiesen and Passell, 1976; Orsagh, 1973). However, in these analyses, lagged effects are absorbed into later measures of enforcement and crime. If these effects are of opposite sign, as we have suggested the effect of crime on enforcement is expected to be, they will tend to cancel and lead to an underestimation of the influence of crime on enforcement. To avoid this possible source of bias, a model of the effects of crime on enforcement must include both short-term and long-term effects.

A second difficulty is also inherent in this approach. To separate the two influences of the rates from one another, we must include some outside predictor variables in the model and use them as instruments, by fixing a priori the partial regression coefficients of the predictors with the criterion score at some prespecified value.[2] However, this procedure is highly sensitive to misspecification; should these identification restrictions be in error, the estimation of effects will again be biased, perhaps grossly (Fisher and Nagin, 1978; Greenberg, 1977).
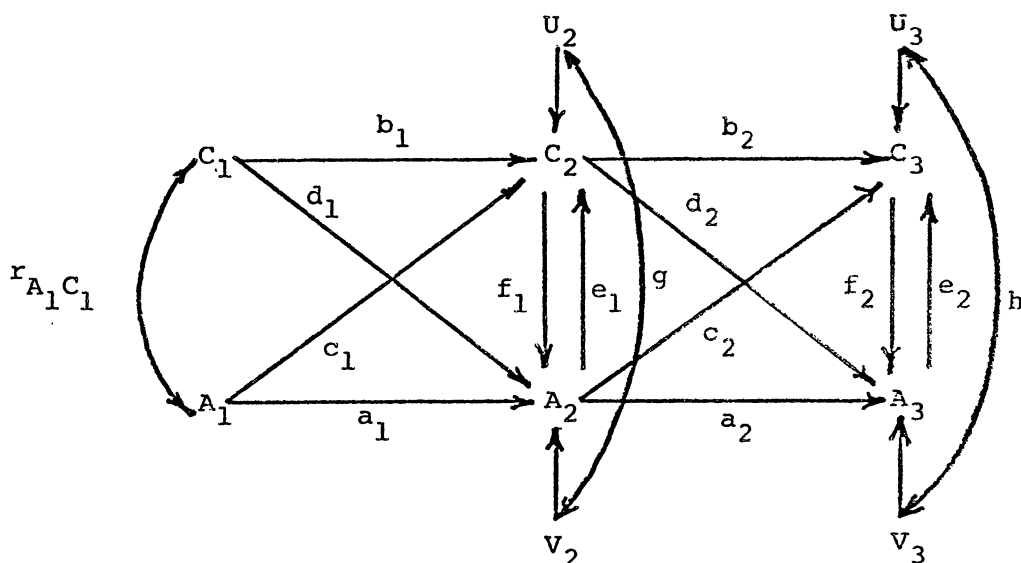
A second approach to the disentangling of reciprocal effects has been to analyze time series data for crime rates, enforcement variables, and a variety of additional variables assumed not to be influenced by crime rates or arrest rates, for a single unit of analysis, usually the entire nation (Votey and Phillips, 1974; Ehrlich, 1975; Land and Felson, 1976). This approach allows explicit consideration of both short-term and long-term effects, both of crime on enforcement and of enforcement on crime, and is therefore superior to cross-sectional analysis. Yet, since it is still necessary to make use of instrumental variables to obtain a unique solution, parameter estimates are subject to the same possible source of bias as estimates derived from cross-sectional analyses.

A third approach to the problem of reciprocal effects, which we extend below, is the use of panel models. Here data are collected for a number of spatially distributed units (e.g., cities, states) at more than one time, and the reciprocal short-term and long-term effects of crime and enforcement estimated. Thus far, all panel studies of crime rates have involved only two waves of data (Tittle and Rowe, 1974; Logan, 1975; Pontell, 1978). With this limited amount of data, however, the utility of panel models is restricted, for it is necessary to impose identification restrictions that are hardly less implausible than those required in cross-sectional and time-series models[3] (Duncan, 1969).

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[2] In practice this is usually done by assuming that some demographic, socioeconomic or political variables are causes of punishment, but do not affect crime either directly or indirectly (except through their influence on punishment). The assumption that a given exogenous variable has an effect of specified magnitude on the crime rate (including zero effect) should be made on the basis of prior knowledge concerning the causes of crime. Yet this knowledge is still primitive and uncertain. Thus any conclusions derived on the basis of such restrictions must be regarded with caution. Nagin (1978) and Fisher and Nagin (1978) have argued on the basis of their review of the literature that the restrictions which have been used to achieve identifiability are in many instances quite implausible.

[3] For example, in an analysis of city crime and arrest rates in Florida for 1971 and 1972, Tittle and Rowe (1974) estimate two-wave panel models which assume that cross-instantaneous effects vanish. Logan (1975), analyzing state index offense and arrest data for the years 1964–1968, also uses two-

[a] Lower case letters represent standardized regression coefficients and correlations among residuals.
Figure 1. Three Wave, Two-Variable Panel Model for Crime Rates and Clearance Rates[a]

A multi-wave panel model. In view of the uncertainty surrounding the assumptions necessary to identify the models reviewed above, the development of alternative methods of analysis seems warranted. Here we explore an approach that we regard as particularly promising—the use of multiwave panel models which impose assumptions about the consistency of effects rather than assume that certain effects have given values. This method permits the data to play a larger role in the analysis, and therefore seems preferable to methods that require stronger assumptions, given the limitations in our knowledge of crime causation and the duration of causal effects.

The basic ideas of the consistency procedure can be seen in Figure 1, which illustrates a three-wave, two-variable panel model. In this model, the per capita crime rate (C) at time t is assumed to de-

pend linearly on the crime rate at time $t-1$, and on the arrest clearance rate (A) at times t and $t-1$. Similarly, the current clearance rate at time t is assumed to depend linearly on the clearance rate at time $t-1$, and on the crime rates at times t and $t-1$. Thus the model includes both lagged and instantaneous reciprocal influences between the crime rate and the clearance rate. The relationship between the crime rate and the clearance rate at time 1 is taken into account but is not subjected to causal analysis. Explicit allowance is made for the possibility that the residuals for the crime rate and clearance rate at time t (U_t and V_t, respectively) are correlated.

Given our present state of knowledge, a model of this type seems highly plausible. The effect of arrests on crime should persist no more than a few years, since rational criminals will attend to recent police performance in evaluating their chances of arrest. Similarly, the effect of crime on arrests should not last longer than a few years, since increased efficiency measures, to the extent that they are influenced by earlier concern with increasing criminal activity, will probably be responsive to short-term rather than long-term concerns.

The constraints in the model consist of

wave models, and studies various specifications. In one of these, lagged effects are assumed zero; in another, only instantaneous effects of crime and only lagged effects of arrest are assumed; in a third, lagged effects of crime are assumed zero and disturbance terms are assumed uncorrelated. Although there is good overall consistency among the estimates obtained on the basis of these various specifications, the results depend on the untested assumptions of the various models.

the assumptions that the autoregressive and cross-coefficients linking the variables between times 2 and 3 are equal to those between times 1 and 2 (in the notation of Figure 1, $a_1 = a_2$, $b_1 = b_2$, etc.). A similar assumption is implied whenever an equation estimated for one time period is used to predict a crime rate or an arrest rate at a later period. With the assumption that these coefficients remain constant, this particular model is overidentified with 6 degrees of freedom, and each of the parameters is individually identified or overidentified.

A major advantage of this specification over those reviewed above is that it allows the data to play a larger role in estimation. Apart from our assumption that variables at time 1 do not influence those at time 3, we do not fix any of the main parameters to prespecified values, but rather make the assumption that the coefficients, whatever they are, remain constant over time.[4] In addition, the constrained models give us a good deal of flexibility in respecification. For instance, it is possible to relax the assumption of constant autoregressive effect ($a_1 = a_2$, $b_1 = b_2$ in Figure 1) and the assumption that time 1 rates have no direct effects on time 3 rates and still just identify the model as a whole. Finally, by introducing more waves of data, it is possible to relax some of the constraints, thus permitting a partial test of these assumptions. Since we had seven waves of data (for the years 1964–1970) available, we were able to take advantage of this flexibility in our analysis.

Data and Procedures

Data. Our analysis of the relationship between crime rates and clearance rates is based on reported index offense rates and arrest clearance rates for 98 U.S. cities for the years 1964–1970. Two cities were dropped because of missing data from an original sample of 100 cities drawn by a proportional stratified random procedure from the total universe of cities with a population over 25,000 for which the F.B.I. had crime and arrest data. The 1970 edition of the F.B.I.'s *Uniform Crime Reports* indicates that the universe from which our sample was drawn represents over 90% of the urban population of the U.S.[5]

Data on population size, number of offenses known to police, and number of crimes cleared by arrest were supplied by the F.B.I. for each of the cities for each year from 1964 to 1970, for each of the seven index felonies. For each city, year and felony, *crime rate* was defined to be the number of offenses known to police for that year divided by the population estimate for that year; and *clearance rate* was defined as the ratio of offenses cleared by arrest to the number of offenses known to the police for that felony and that year.

Procedures. Although theoretical considerations suggest that lagged causal effects may exist, theory does not tell us precisely how long a lag to expect. Consequently we examined models with lags of one, two and three years. In models with one-year lags, correlations among the crime rates for consecutive years were so high that the correlation matrix could not be inverted. Under this circumstance, it is not possible to solve for the individual regression coefficients. Substantively, this means that too little change in the rates occurs over the period of a single year for us to detect accurately the impact

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[4] For a number of our specifications, we estimated models utilizing both unstandardized (metric) and standardized variables. The first set of models could not fit the data, while the latter set provided good fits. We are not completely certain of the reason for this, but suspect that it is a consequence of trends in the data. Over the years 1964–1970, reported crime rates rose steadily (e.g., total index offenses increased by 101%) while clearance rates experienced no comparable trend. Under this circumstance, a model that assumes constant metric coefficients would not be expected to provide a good fit; on the other hand, a model based on standardized variables avoids this difficulty by effectively detrending the data.

[5] *Uniform Crime Reports* lists cities for which crime rate data are reported according to four strata by population size: 25,000–50,000; 50,000–100,000; 100,000–250,000; and over 250,000. Using the list of cities in the 1968 edition of *Uniform Crime Reports,* we randomly selected 53, 29, 11 and seven cities from each of these strata, respectively. Those numbers are proportional to the number of cities in each stratum of the *Uniform Crime Report's* list of cities. They were drawn to total 100, because that is the number of cities for which the F.B.I. indicated a willingness to supply arrest data.

of a predictor on this change. Thus, if there are lagged effects of any substantial magnitude, they must involve lags of more than one year. When a time lag of two years was employed, the correlations, though still high, were no longer too high to permit matrix inversion. Changes in rates over a two-year period were more substantial, and consequently more discernible. Models with three-year lags yielded results that were similar to those with two-year lags, and are not reported here.

With the exception of the crime rate and clearance rate at time 1, no exogenous variables are explicitly introduced into any of these models. In order to deal with the problem of misspecification and spuriousness, it would have been preferable to have had such controls, but thus far we have been unable to develop adequate data of this type. We note, however, that the problem of bias resulting from the omission of exogenous variables should be less serious in a panel model than in a cross-sectional analysis, as earlier values of C and A are controlled, and this will partly control for the effect of exogenous causes of C and A.[6]

A series of specifications, all generalizations of Figure 1, were estimated. The best fit was obtained by a four-wave model (1964, 1966, 1968, 1970) in which the standardized crime and clearance rates at time t were assumed to depend linearly on one another at times t and $t-1$,

and all autoregressive coefficients were treated as free parameters. Cross-coefficients linking time 4 with time 3 were constrained to equal those linking time 3 with time 2, but no constraints were imposed on the coefficients linking times 1 and 2. This procedure permits a partial test of the assumption that coefficients remain constant over time. The coefficients representing instantaneous influences were all constrained to remain constant; serial correlation among error terms was assumed to be absent, but correlations among contemporaneous error terms were estimated.[7]

Results. We used LISREL III (Jöreskog and Sörbom, 1975) to estimate all the models discussed below. This procedure provides maximum-likelihood estimates of regression coefficients in overidentified models and also has an option for imposing the constraints that characterize the models. The fit of a model to the observed data is evaluated by comparing the observed matrix of correlations among the crime and clearance rates with the matrix predicted by the model using the parameter estimates generated by the maximum-likelihood procedure. This evaluation was done both by inspection and by use of an appropriate chi-square goodness-of-fit test statistic with degrees of freedom equal to the number of overidentifying restrictions in the model.

We estimated eight applications of this model, one for each of the seven index offenses and one for total index crime. In only one instance (rape) was there a statistically significant difference between the observed and predicted correlation matrices at the 0.05 level (for the other categories, the probability of obtaining a discrepancy as large as the one observed ranged from $p > 0.50$ for total offenses to $p > 0.95$ for burglary), and here the sub-

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[6] The two-variable model will provide unbiased estimates in the face of exogenous causes of C and A when the exogenous variables have created a spatial distribution of crime rates and arrest rates but no longer influence those rates; and also when the effect of the exogenous variables persists provided that lagged cross effects vanish beyond a certain point and the exogenous variables remain constant in time. In the latter case, the panel model effectively controls for omitted exogenous variables even if it is not known what they are or exactly how they influence the dependent variables. Since most of the variables that are expected to influence crime rates and clearance rates (e.g., demographic, socioeconomic, political and cultural variables) do not change much from one year to the next, we believe the assumption of constancy should be fairly safe so long as the data are collected over a limited time span. For a complete discussion of issues related to the question of bias in this model, see Greenberg and Kessler (forthcoming).

[7] Our estimates would be biased in the presence of serially correlated errors. To check that this was not a problem, we made use of the fact that our models can be identified in the presence of first order serially correlated errors using only time 1 variables as instruments. We found that the parameter estimates obtained for total offenses by this procedure differed only in the third decimal place from those obtained on the basis of the assumption that serial correlation of errors is absent.

Table 1. Parameter Estimates[a] for Panel Model for F.B.I. Index Offenses[b] (N=98)

	Offense							
Coefficient	Murder	Rape	Aggravated Assault	Robbery	Burglary	Grand Larceny	Auto Theft	Total
1. A→C								
A_1C_2	− .033	.058	− .034	− .025	− .005	.078	− .013	.121
$A_2C_3=A_3C_4$	− .122	.026	− .113*	− .002	− .014	.064	.015	.151
A_tC_t	.380*	− .069	.129	− .068	− .056	− .115	− .082	− .260
2. C→A								
C_1A_2	.508	.004	− .398	− .375	.442	.053	− 1.528	.185
$C_2A_3=C_3A_4$	.313	− .063	− .338	− .393	.418	.130	− 1.439	.177
C_tA_t	− .415	− .002	.598	.241	− .578	− .211	1.510	− .283
3. C↔A								
C_1A_1	.537*	.309*	− .119	− .098	− .255*	− .451*	− .456*	− .346*
U_1V_1	.279	.235	− .330	− .021	.091	.123	− .218	.195
U_2V_2	.151	.166	− .421	.025	.101	.006	− .265	.136
U_3V_3	.094	.140	− .567	− .062	.227	.041	− .174	.178
χ_6^2	3.17	12.61	3.43	2.52	1.24	3.60	3.40	4.57
Probability Level	.75	.025	.75	.75	.975	.50	.75	.50

[a] X_iY_j denotes the standardized regression coefficient for the causal effect of variable X at time i on variable at time j. The 95% confidence limits for starred coefficients do not include zero.

[b] See text for description of the model.

stantive discrepancy between the two matrices was quite small, with a mean absolute discrepancy between elements of only 0.02 and only two discrepancies larger in magnitude than 0.10. In light of this correspondence, we interpreted the parameters estimated by this model for all eight crime categories.

Table 1 presents the parameter estimates for the eight versions of this model, one for each index offense and one for total crime. The results are presented in three panels, corresponding to (1) the effects of arrest on crime, (2) the effects of crime on arrest, and (3) unanalyzed cross-sectional relationships.[8] These are discussed in turn.

Panel 1 provides information about the impact of arrest rates on crime. Of the 24 coefficients, only two have 95% confidence limits that do not include zero. The only lagged coefficient that is significantly negative occurs for assault, but it is quite small in magnitude (−.113) and is not stable over time; the only instantaneous coefficient whose 95% confidence limits do not include zero is the coefficient for homicide, and it is *positive*, though only moderate in size (0.38).

The second panel provides evidence concerning the effect of crime on arrests. Coefficients for the instantaneous and lagged effect of crime on arrests are moderate to substantial for all offenses but rape. In the case of murder, burglary, larceny, and total offenses the instantaneous effects are negative, while for assault, robbery and auto theft they are positive. However, the standard errors for these coefficients are large (in general, standard errors for the clearance rates are larger than those for the crime rates), and none of the coefficients achieves statistical significance at the 0.05 level.

Finally, panel 3 shows the cross-sectional exogenous correlations among the crime and arrest rates at time 1 for each offense, and the cross-sectional correlations among the error terms for the offenses. Examining the former, we see that six of the eight correlations are statistically significant; those for murder and rape are positive, while those for burglary, larceny, auto theft and total offenses are negative. In a cross-sectional bivariate analysis these correlations would have been taken as evidence of the effect of arrests on crime, an interpretation which our results call into question.

The correlations among the residual error terms tell us how well our model has done in accounting for the cross-sectional

[8] As the estimates of stability parameters are of less interest they are not presented here, but will be furnished by the authors upon request.

correlation over each of the subsequent time points of the panel. These correlations measure the influence of exogenous variables on both rates. If the correlated error terms were comparable to the exogenous cross-sectional correlations, it would indicate that the cross-sectional correlations are primarily due to unconsidered exogenous variables. This is only partly the case in our data. With the exception of assault, the correlated error terms are smaller than the exogenous correlations, though in some instances not entirely negligible. However, none of these correlations is statistically significant. Taking this finding, together with the absence of evidence for substantial cross-effects between crime rates and clearance rates, we see that much of the cross-sectional correlations are due to the internal stabilities of the rates themselves, and the initial correlations among these rates, which have not been analyzed. In our data, then, initial correlations brought about by historical forces that no longer operate, together with the natural changes in the rates themselves and the effects of omitted exogenous variables (e.g., socioeconomic and demographic variables), account for the major part of the cross-sectional correlations at later points in time. The reciprocal influences of crime on arrests and arrests on crime are negligible by comparison.

On the basis of these findings we estimated a model in which all autoregressive effects were estimated, but *all* cross effects were fixed at zero. The probability that the discrepancies found between observed and expected correlations could have arisen by chance was greater than 0.20 for homicide, assault and auto theft; greater than 0.30 for rape and robbery; greater than 0.50 for total offenses; greater than 0.70 for burglary; and greater than 0.80 for larceny. On the basis of this test and the tests for the individual parameters in Table 1, we conclude that none of the cross effects in our original model is significantly different from zero. Aggregate criminal activity for the F.B.I. index offenses is not substantially influenced by marginal variations in arrest clearance rates within the range found in our sample of American cities.

Discussion

Although our finding that arrest rates have no measurable effect on reported crime rates is contrary to the deterrence hypothesis, there are a number of ways to interpret our findings consistent with the deterrence doctrine. One possible explanation lies in the interdependency of criminal justice sanctions. Arrest is not a "pure" sanction; an individual who is arrested faces a stochastic distribution of dispositions ranging from dismissal of charges to conviction and imprisonment and, for some offenses, execution. If prosecutorial and judicial agencies respond with greater leniency to the caseload pressures generated by higher arrest rates, or if higher arrest rates are achieved by arresting suspects on the basis of weak evidence that will not stand up in court—with more dismissals as the consequence—the net effect could be to nullify any crime prevention effect due to arrests alone.

Another possible explanation for this seeming insensitivity of crime rates to changes in the clearance rate is that prospective offenders are for the most part ignorant about marginal changes in the probability of being arrested after involvement in an offense. Unless information about sanctions is communicated to potential offenders, variation in sanctions cannot be expected to deter (though it may influence crime rates in other ways, such as through incapacitation, if sanctions entail incarceration).

It is also conceivable that for some offenses, the consequences of an arrest for most of those arrested are not sufficiently serious to make an arrest an effective sanction; or that the stigmatizing effects of an arrest nullify any crime-prevention effects.

The cross-sectional correlations between clearance rates and crime rates in our data are comparable to those reported in the studies of crime rates that have appeared over the last decade (Tittle and Rowe, 1974; Logan, 1975; Brown, 1978). We therefore conclude that these studies, which have been interpreted as lending support to the deterrence doctrine, do not do so. Our analysis strongly suggests that the correlations interpreted in these studies as evidence of crime deterrence

may in fact have been spurious. Only through the use of statistical procedures that are capable of disentangling the various causal effects that are expected on theoretical grounds to link crime rates and punishment levels is it possible to draw inferences about the effect of punishment on crime. Previous studies of crime rates have failed to do this.[9]

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[9] This observation is applicable not only to research that has analyzed aggregated crime rates, but also to studies of the relationship between self-reported criminal activity and perceptions of risk, as perceived risk could be influenced by participation in illegal activity, as well as a cause of it.